

FURTHER COMMENTS ON THE PROPOSAL TO VALIDATE *CARDIUM CALIFORNIENSE* DESHAYES, 1839. (MOLLUSCA, CARDIIDAE).

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(see vol. 31, p. 238 and vol. 32, p. 204)

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There are a number of aspects of this case which to me are unsatisfactory, either in the particular proposals of the application or in the general application of the Code to such cases involving subjective synonymy. In a hypothetical example involving the three nominal taxa *A-us x-us* Smith, 1800, *A-us y-us* Brown, 1810 and *A-us z-us* Jones, 1820, suppose authors have consistently regarded *A-us y-us* as a junior subjective synonym of *A-us x-us*. If Robinson now declares that, contrary to all previous opinion, *A-us y-us* is really an unused senior synonym of *A-us z-us*, it is neither reasonable nor logical for him to cite ten uses of *A-us z-us* (in compliance with Article 79b) as supporting evidence for the suppression of *A-us y-us*. Surely Article 79b can be invoked only in cases of unused senior synonyms where the identity of the senior name has already been accepted as a synonym of the junior name for which general usage is claimed.

In the present case involving *Cardium ciliatum* Fabricius, 1780, *C. boreale* Broderip & Sowerby, 1829 and *C. californiense* Deshayes, 1839, *C. boreale* was regarded as a junior synonym of *C. ciliatum* by Dall, 1901, and subsequently by Grant & Gale, 1931, Clench & Smith, 1944, and Abbott, 1974. I know of no suggestion, prior to Kafanov's application, that *C. boreale* might be a synonym of *C. californiense*. Even in Kafanov's own taxonomic paper (1974) on *Clinocardium*, in which he might have been expected to present detailed evidence for such a synonymy, there is no mention of *C. boreale*.

In the absence of type material, Kafanov's claim for the synonymy of *C. boreale* with *C. californiense* rests solely on the basis of the original description, particularly the words 'numerous close-set rounded ribs'. According to Kafanov, *C. ciliatum* has 'relatively sparse radial ribs', but as the usual number is 32 to 38 they could nevertheless quite reasonably be described as numerous. A comparison of the two species as illustrated by Grant & Gale (1931, pl. 19) shows *C. ciliatum* (fig. 11) with about 36 ribs and an Alaskan specimen of *C. californiense* (fig. 16) with the same number. Habe & Ito (1965, pl. 44, figs 2, 3) illustrate Japanese specimens of the two species each with about 32 ribs, those of *C. ciliatum* being more closely-set than those of *C. californiense*. The described shape of the ribs—'rounded'—does suggest *C. californiense* rather than *C. ciliatum*, although this character varies with size, age and degree of wear.

In *C. ciliatum* the angularity of the ribs decreases considerably from anterior to posterior (Grant & Gale, 1931, pl. 19, fig. 11; Abbott, 1974, p. 487, fig. 5583). Dall (1907), when describing the related *C. fucanum*, comments on 'the angulation... which is characteristic of *C. ciliatum* in the young stages', implying that this is less evident in older shells. The original description states: 'the two ends nearly equal in length, the posterior being slightly angulated'. *C. ciliatum* is the more equilateral of the two species. In my opinion the original description of *C. boreale*, with the single exception of the words 'rounded ribs' fits *C. ciliatum* at least as well as *C. californiense* and, from the stated dimensions

of length 1.6 inches, height 1.3 inches, is possibly even more appropriate for *C. fucanum*, a species known from the southern Bering Sea but not so far recorded from the area of Icy Cape.

All this, however, is a matter of subjective taxonomic judgement. What concerns me is that Kafanov's argument for the synonymy of *C. boreale* with *C. californiense*, on which his request for the suppression of *C. boreale* is based, has not been subjected to critical appraisal as it has not been published anywhere other than in the application itself. Is it not premature for the Commission to take any action in a case requiring the use of plenary power in which the reasons for the proposals derive, in the absence of any type material, entirely from unproven assumptions made by the applicant and which are contrary to all previous interpretations? Would not any action taken by the Commission seem to endorse Kafanov's 'assurance' and so prejudice any future work on the taxonomy of *Clinocardium* species from an as yet poorly investigated faunal area?

As to the question (if the application is accepted) of whether *C. boreale* Broderip & Sowerby should be suppressed for both priority and homonymy, or priority only, there is clearly a misunderstanding here. For at least as long as *C. boreale* Broderip & Sowerby has been accepted as a junior synonym of *C. ciliatum*, *C. boreale* Reeve, 1845, has been accepted as a junior synonym of *C. groenlandicum* Bruguière, 1789. Its validity is thus not affected by any decision concerning *C. boreale* Broderip & Sowerby, except that if the latter were suppressed for homonymy as well as priority it is open for someone to resurrect *C. boreale* Reeve by claiming it not to be a junior synonym after all. The reply by Kafanov, in response to the question raised by Professor Mayr, that it is not his intention that *C. boreale* Reeve should remain invalid, is therefore misleading, and the Commission would avoid possible future problems by considering only the original proposal to suppress *C. boreale* Broderip & Sowerby for priority but *not* for homonymy. To do otherwise would be contrary to the guiding principles concerning the use of plenary power as expressed in Article 79a(ii).

(2) By the Secretary (*International Commission on Zoological Nomenclature, c/o British Museum (Natural History), Cromwell Road, London SW7 5BD*)

This application was sent out to the Commissioners for voting on by March 1980 in V.P.79(25). Commissioner David Heppell voted against the proposal, sending in the above comment which was immediately sent to Dr Kafanov. Unfortunately, despite letters sent to him following up this additional information, we have not received a reply. I have myself had another look at the application and can see nothing in it to suggest that the synonymy of *C. boreale* with *C. californiense* is of long standing and it would appear that, although Dr Starobogatov supports Dr Kafanov in this application, no other malacologist has drawn our attention to the point.

I propose, therefore, to close this case unless I hear to the contrary within six months of the publication of this Bulletin.

Dr Myra Keen assures me that the citation of her name in support of Dr Kafanov must be due to a misunderstanding or a mistranslation of her. In fact she shares Mr Heppell's view of the case.